of a book such as this, and in its publication Mr. Crookes has rendered an important service to the analytical chemistry of the day. The author's connection with the *Chemical News* has doubtless afforded him great and peculiar advantages in the compilation of the materials for his work; indeed, we notice that not a few of the most valuable processes he describes have already appeared in that journal.

Not the least admirable feature in the book (and herein it differs from the ordinary run of quantitative manuals) is the prominence given to methods for detecting and estimating the so-called "rare" metals. Thus we have methods given for the extraction and quantitative separation of lithium, cæsium, and rubidium; cerium, lanthanum, and didymium; glucinum and yttrium, &c. Bunsen's method of analysing platinum ores is also fully described. The discoverer of thallium may justly say that if investigators were more in the habit of looking for the "rare" elements, they would doubtless turn up unexpectedly in many minerals.

But for fear that some "practical man" has already made up his mind about the character of this book, we hasten to say that by far the larger portion of it is devoted to the analytical processes connected with the more important metals and their ores. The sections on iron and copper are particularly complete, all the newest and best methods being minutely described. We may instance Matthiessen's process for the preparation of pure iron, the Mansfield method of copper assay, and Meunier's methods for the immediate analysis of meteoric iron. Under the article "Soda-ash," attention is very properly directed to the absurdity practised in the alkali trade of at one time using the old atomic weight (24) of sodium, and at another the real number in stating the value of soda-compounds, the manufacturer invoicing the strength of his ash in accordance with the basis of calculation which he knows will be employed in reporting on its quality. This custom in many cases amounts to a positive abuse, and is a constant source of vexatious complaint between buyer and seller. The author quotes an instance in which soda-ash of identical quality has been known to be invoiced, part to one customer as containing 48 per cent., part to another as 49 per cent., and part to a third as 50 per cent.; the actual percentage being 48¼; the separate consignments being reported also of these different strengths by the analysts in the different towns to which the goods were sent. Inasmuch as soda-ash is usually valued at so much per cent., this amounts to a fraud on the purchaser. Surely the Alkali Manufacturers' Association would consult their real interest by putting an end to such a petty sham as this. But after all this is only on a par with the iniquitous system of "high" and "low" analyses, which is a scandal to the chemistry of the day. *Apropos* of this we would add that those interested in the agitation which has arisen in certain chemical quarters respecting the proper methods for estimating superphosphates and phosphoric acid generally, will find ample details in this work of the reliable processes which have hitherto been devised to that end. Under the head of carbon the method of assaying animal charcoal is described, and this includes an account of Schiebler's calcimeter and the mode of using it; the account of Heinrich's inquiry into the methods for the proximate analysis of coal, which is here introduced, together with the blowpipe assay, constitute one of the most complete things in the book.

In the midst of so much that is excellent it may seem invidious to seek for real or supposed omissions, but we venture to think that a few addenda in a future edition would add to the value of the book. For example, Bunsen's method of preparing pure platinum tetrachloride from scrap platinum, and his simple and expeditious method of recovering this metal from the residues in the process, might be an advantageous addition to the condensed description by Messrs. Teschemacher and Denham Smith of the ordinary method of estimating potash. Without doubt not a few of the errors to which a potash determination made by this method is liable are due to the use of impure platinum. A chapter devoted to the description of useful or improved forms of apparatus, such as the new filter-pump, would also form a valuable addition. Still we must not forget to add that Dr. Carmichael's neat and ingenious method of analysis receives its due share of attention. Nor do we see the reasonableness of making the selection of methods strictly inorganic; for surely the modifications in the process of ultimate analysis introduced by Mr. Warren cannot constitute the sum of the improvements in organic analysis, proximate and ultimate, which have come under the author's notice.

But we have said enough to indicate the character and scope of this work, and imperfect as our sketch is, it will at least serve to show that the book admits of very general application. It will doubtless attain to the popularity which it merits, and the chemical community will thank the author for the worthy contribution he has rendered to its literature.

T. E. THORPE

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Sun

IN NATURE for May 18, I find a review by Prof. Newcomb of my book on the Sun, and I beg leave to give some explanations respecting it. It is a pity that Mr. Newcomb has received my book only so lately (about one year after the printing was finished). On this account the criticism which he makes respecting spectrum analysis and the defects of the work on that head is entirely admissible; but this is no fault of the author. Since the book was written sixteen months have elapsed, and during this time great progress has been made in this branch of science, and nobody knows it better than myself, since I find so many things to add to that chapter.

But the criticism seems not so just when he reproaches the work with appearing to undertake to spare the reader the trouble of having recourse to elementary books. At the time of writing, neither of the valuable books which we have now by Roscoe and Schellen were published or had reached me, and the common treatises are very scanty in this respect, so that it was indispensable then, and perhaps even now, to indicate to the general reader the principles of the new science.

In the rapid progress of discovery, a book becomes old very soon, and a reviewer must not forget that printing requires time, and that this work appeared at the very moment of the breaking out of the war, so that it was shut up in Paris for almost another six months.

But a more serious criticism is that which refers to the temperature of the sun. As this is a repetition of M. Zöllner's critique, I beg to give some explanation. It is true that I have assumed that radiation is proportional to temperature without regard to the law of Dulong and Petit, and to the condition of

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the surface of the body. I did not, however, ignore that law, and I have taken it into account when speaking of Prof. Kirchhoff's discoveries. But here it was quite out of place to refer to it. Indeed, the consequence of this law is that a body may have a very high temperature, and yet radiate but very little; but the law does not state that a body may radiate more than its own temperature allows. When we judge, therefore, of the temperature from the radiation, we certainly commit an error; but so we always judge the temperature from one part of the effect that it is capable of producing; and taking into account the law of Dulong, we should find even a higher temperature in reality in the radiant body, as is the case with the gases.

The conclusion, therefore, to which I have arrived, after Mr. Waterston, is, I think, by no means excessive, but if there is an objection possible to be made, it is exactly in the direction opposite to that of my reviewer. Certainly this conclusion is at variance with that of M. Zöllner, but it agrees with the results of other observers. This high temperature besides is really a virtual temperature, as it is the amount of radiation received from all the transparent strata of the solar envelope, and this body at the outer shell must certainly be at a lower temperature.

But this does not prove the incorrectness of my proposition that a thermometer dipped inside the solar envelope in contact with the photosphere, would indicate the enormous temperature that Mr. Waterston has found for the first time.

P. A. SECCHI,
Director of the R. College Observatory

WILL you permit me to make a few comments on Prof. Newcomb's review of my treatise upon the Sun.

Soon after the work had appeared, I was informed that the account I had given of Mr. Stone's treatment of the transit observations in 1769 was not such as Prof. Newcomb would admit to be just. Knowing how much attention Prof. Newcomb has given to this subject, and his great skill as a mathematician, I was prepared to learn that I had misapprehended some points of the discussion between himself and Mr. Stone. I do not even now know to what specific statements of mine he objects; but he may rest assured that my sole object has been, and is, to give a just account of the matter. My account is in agreement with that given by Sir John Herschel, and by Admiral Manners when the Gold Medal of the Royal Astronomical Society was presented to Mr. Stone. I had also inferred from the nature of the discussions between Mr. Stone and M. Faye, and between Mr. Stone and Prof. Newcomb, that the truth lay much as in my narrative. At any rate, those who were present at the meetings of the Royal Astronomical Society in 1869 and 1870 will scarcely think that I have been led by any personal prejudices to advocate Mr. Stone's cause with undue favour, for Mr. Stone's strictures on some of my papers, and especially on papers relating to the subject of the transits of Venus, were severe even to bitterness.

I believe that Prof. Newcomb's mastery of this special subject is calculated to prevent him from rightly judging my treatment of it. He sees it from too near a stand-point, and therefore unduly enlarged. I am sure that on a careful reconsideration of the matter he will feel that I could not have given a fuller account of it than I have, without spoiling the symmetry of my book. Already a seventh part of the letter-press and more than a third of the appendix (besides three plates and twenty-four diagrams) have been given to the subject of the sun's distance. I do not think that more space could very well have been spared. It remains yet to be proved that a single statement in these pages is inaccurate. I deny confidently that the distortion of the limbs of Venus and Mercury in transit has been *proved* to be the product of insufficient optical power. Irradiation must produce such effects to a greater or less extent; and I renew "gravely" my proposal to measure the effect, whatever its cause or causes, during the next transit. I would remind Prof. Newcomb that every observer at Greenwich noticed the effect (more or less) during the transit of Mercury in 1868. Now the Greenwich instruments are not commonly supposed to be utterly imperfect, nor the Greenwich observers wholly unskilled. Even if we admitted both these points, I should still adhere to my proposal. For I have shown in the Monthly Notices of the Royal Astronomical Society for 1870, that those two observations which differed most widely are brought into perfect agreement when the relative breadth of the black ligament observed in each case is taken into account.

Prof. Newcomb has misunderstood my remarks about the D line in the spectrum. I have never concluded, and certainly I have nowhere stated, that "the light of the sodium lines proper is reduced." What I have pointed out is that where those lines fall on the spectrum of the electric light, and where, therefore, we should expect an increase of light, there seems to result darkness. In p. 118 I am careful to use in one place the word *seem*, in another the word *appear*.

I must remind Prof. Newcomb that three countrymen of his own, Professors Harkness, Curtis, and (quite recently) Young, have supposed, with me, that the theory *has* been maintained, that the light of the corona is due to sunlight directly illuminating our atmosphere, and that they and Mr. Baxendell have opposed that theory as pointedly as I have.

I have, however, to admit that some passages "indicating personal feeling" would have been better—much better—omitted. I should have remembered that the explanation of such personal feeling would be unknown to most of my readers. Those who know that because I advocated opinions respecting the corona, which are now all but universally admitted to be just, I was spoken of as "simply making myself ridiculous," will at least acquit me of responding as rudely as I had been attacked. But the generality of my readers had heard nothing of this and other assaults upon me.

I take this opportunity of noting that Dr. Armstrong, of the London Institution, has shown me that in my account of the researches of Mr. Lockyer and Dr. Frankland I have not done the former justice. Some alterations must be made also in my narrative of the work of Dr. De la Rue and P. Secchi in Spain in 1860; much more of the credit of the results then obtained being due to Dr. De la Rue than I had judged from the narrative in P. Secchi's "Le Soleil." Also the enunciation of the aurora theory of the corona must be assigned to Prof. Norton of America.

RICHARD A. PROCTOR

[With respect to the penultimate paragraph of the above letter, we need only refer to our own comments on two previous letters from Mr. Proctor, under date July 7 and August 4, 1870, which we now reprint. ED.:—"For an accurate though incomplete statement of Dr. Frankland's and Mr. Lockyer's theory of the Corona, we refer our readers to the first number of NATURE. Many of them will not be surprised to find that it is *not* what Mr. Proctor states it to be. Dr. Frankland and Mr. Lockyer, from their laboratory experiments, have shown that the pressure at the base of the chromosphere is small, and they have therefore stated that it is scarcely possible that a very extensive atmosphere lies outside the chromosphere. Mr. Lockyer has shown, moreover, that the height of the chromosphere as seen by the new method probably falls far short of its real height as seen during an eclipse as it was seen by Dr. Gould. A reference to the same number of this journal will also show that Mr. Proctor has misrepresented Dr. Gould's statements, which endorse the idea put forward by Dr. Frankland and Mr. Lockyer. Dr. Gould has expressly stated 'that there were many phenomena which would almost lead to the belief that it was an atmospheric rather than a cosmical phenomenon.' This is an opinion held by Faye and other distinguished astronomers, and Mr. Lockyer has simply shown that should this turn out to be the case, the continuous spectrum observed may be explained. Astronomers did not require Mr. Proctor to tell them what he has recently been enforcing; but, more modest than he, they have been waiting for facts, and Mr. Proctor surely is old enough to see that by attempting to evolve the secrets of the universe, about which the workers speak doubtfully, out of the depths of his moral consciousness, he simply makes himself ridiculous, and spoils much of the good work he is doing in popularising the science."—"Still holding to our comments, we gladly state that they were not written in the spirit in which Mr. Proctor has read them. He is known to all as an astronomical worker, and our objection to his mathematical result was that it was based upon data among which the principal point at issue was accepted as proved."]

Rain after Fire

IN Paris, on Wednesday the 24th inst., after describing the terrible conflagrations, one of the correspondents writes thus:—

"A more lovely day it would be impossible to imagine, a sky of unusual brightness, blue as the clearest ever seen, a sun of surpassing brilliancy even for Paris, scarcely a breath of wind to ruffle the Seine. Such of the great buildings as the spreading

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